

AN
ACCOUNT

OF A

NEW EUDIOMETER.

By Mr. CAVENDISH, F.R.S.

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A N A C C O U N T, &c.

DR. PRIESTLEY's discovery of the method of determining the degree of phlogification of air by means of nitrous air, has occasioned many instruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé FONTANA is by much the most accurate of any hitherto published. There are many ingenious contrivances in his apparatus for obviating the smaller errors which this experiment is liable to; but the great improvement consists in this, that as the tube is long and narrow, and the orifice of the funnel not much less than the bore of the tube, and the measure is made so as to deliver its contents very quick, the air rises slowly up the tube in one continued column; so that there is time to take the tube off the funnel, and to shake it before the airs come quite in contact, by which means the diminution is much greater and much more certain than it would otherwise be. For instance, if equal measures of nitrous and common air are mixed in this manner, the bulk of the mixture will, in general, be about one measure; whereas, if the airs are suffered to remain in contact about one-fourth of a minute before they are shaken, the bulk of the mixture will be hardly less than one measure and two-tenths, and will be very different according as it is suffered to

remain a little more or a little less time before it is shaken. In like manner, if through any fault in the apparatus, the air rises in bubbles, as in that case it is almost impossible to shake the tube soon enough, the diminution is less than it ought to be.

Another great advantage in this manner of mixing is, that thereby the mixture receives its full diminution in the short time during which it is shaken, and is not sensibly altered in bulk after that; whereas, if the airs are suffered to remain some time in contact before they are shaken, they will continue diminishing for many hours.

The reason of the abovementioned differences seems to be, that in the Abbé FONTANA's method the water is shaken briskly up and down in the tube while the airs are mixing, whereby each small portion of the nitrous air must be in contact with water, either at the instant it mixes with the common air, or at least immediately after; and it should seem, that when the airs are in contact with water during the mixing, the diminution is much greater and more certain than when there is no water ready to absorb the nitrous acid produced by the mixture. This induced me to try whether the diminution would not be still more certain and regular if one of the two kinds of air was added slowly to the other in small bubbles, while the vessel containing the latter was kept continually shaking. I was not disappointed in my expectations, as, I think, this method is really more accurate than the Abbé FONTANA's; and, moreover, in the course of my experiments I had occasion to observe a circumstance which is necessary to be attended to by those who would examine the purity of air with exactness by any kind of eudiometer, besides some others which tend very much

much to explain many of the phenomena attending the mixture of common and nitrous air.

The apparatus I use is as follows. A (fig. 1.) is a cylindrical glass vessel, with brass caps at top and bottom; to the upper cap is fitted a brass cock B; the bottom cap is open, but is made to fit close into the brass socket *Dd*, and is fixed in it in the same manner as a bayonet is on a musquet. The socket *Dd* has a small hole E in its bottom, and is fastened to the board of my tub by the bent brass *FfG*, in such manner that *b*, the top of the cock, is about half an inch under water; consequently if the vessel A is placed in its socket, with any quantity of air in it, and the cock is then opened, the air will run out by the cock, but will do so very slowly, as it can escape no faster than the water can enter by the small hole E to supply its place.

Besides this vessel, I have three glass bottles like M (fig. 2.) each with a flat brass cap at bottom to make it stand steady, and a ring at top to suspend it by, and also some measures of different sizes such as B (fig. 3.); these are of glass with a flat brass cap at bottom and a wooden handle. In using them they are filled with the air wanted to be measured, and then set upon the brass knob C fitted upon the board of my tub below the surface of the water, which drives out some of the air, and leaves only the proper quantity. This measure is easier made, and more expeditious in using, than the Abbé FONTANA's, and, I believe, is equally accurate; but if it was not it would not signify, as I determine the exact quantity of air used by weight.

There are two different methods of proceeding which I have used; the first is to add the respirable air slowly to the nitrous; and the other, to add the nitrous air in the same manner to the respirable.

respirable. The first is what I have commonly used, and which I shall first describe. In this method a proper quantity of nitrous air is put into one of the bottles M, by means of one of the measures above described, and a proper quantity of respirable is let into the vessel A, by first filling it with this air, and then setting it on the knob C, as was done by the measure. The vessel A is then fixed in the socket, and the bottle M placed with its mouth over the cock. Then on opening the cock, the air in the vessel A runs slowly in small bubbles into the bottle M, which is kept shaking all the time by moving it backwards and forwards horizontally while the mouth still remains over the cock.

Notwithstanding the precautions used by the Abbé FONTANA in measuring the quantity of air used, I have sometimes found that method liable to very considerable errors, owing to more water sticking to the sides of the measure and tube at one time than at another: for this reason I determine the quantities of air used and the diminution, by weighing the vessels containing it under water in this manner. From one end of a balance, placed so as to hang over the tub of water, is suspended a forked wire, to each end of which fork is fixed a fine copper wire; and in trying the experiment the vessel A, with the respirable air in it, is first weighed, by suspending it from one of these copper wires, in such manner as to remain intirely under water. The bottle M, with the proper quantity of nitrous air in it, is then hung on in the same manner to the other wire, and the weight of both together found. The air is then let out of the vessel A into the bottle M, and the weight of both vessels together found again, by which the diminution of bulk which they suffer on mixing is known. Lastly, the bottle M is taken off, and the vessel A weighed again

again by itself, which gives the quantity of respirable air used. It is needless to determine the quantity of nitrous air by weight; because, as the quantity used is always sufficient to produce the full diminution, a small difference in the quantity makes no sensible difference in the diminution*. In this manner of determining the quantities by weight, care should be taken to proportion the lengths of the copper wires in such manner that the surface of the water in A and M shall be on

* Mr. DE SAUSSURE also determines the quantity of air which he uses by weight; but does it by weighing the vessels containing it in air. This method is liable to some inaccuracy, as the air in the vessel is apt to be compressed by putting in the stopper; though, I believe, that, if care is taken to push in the stopper slowly, the error arising from thence is but small. It is also less expeditious than weighing them under water, as some time is necessarily lost in wiping the wet off the vessels; but, on the other hand, it requires less apparatus, which makes it fitter for a portable apparatus as Mr. DE SAUSSURE's was. If any gentleman is desirous of adapting this method of determining the quantities to the above described manner of mixing the airs, nothing more is required than to have glass stoppers fitted to the vessel A and to the bottle M.

It is needless to mention, that in both these methods no sensible error can arise from any difference in the specific gravity of the air; for the thing found by weighing the vessel is the difference of weight of the included air and of an equal bulk of water, which, as no air is less than 500 times lighter than water, is very nearly equal to the weight of a quantity of water, equal in bulk to the included air.

It must be observed, that a common balance is not convenient for weighing the vessels of air under water, without some addition to it; for the lower the vessel of air sinks under the water, the more the air is compressed, which makes the vessel heavier, and thereby causes that end of the beam to preponderate. This makes it necessary either to have the index placed below the beam, as in many assay balances; or by some other means to remove the center of gravity of the beam so much below the center of suspension as to make the balance vibrate, notwithstanding the tendency which the compressibility of the air in the vessels has to revert it.

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the same level when both have the usual quantity of air in them, as otherwise some errors will arise from the air being more compressed in one than in the other. This precaution indeed does not entirely take away the error, as the level of the water in M is not the same after the airs are mixed as it was before; but in vessels of the same size as mine, the error arising from thence can never amount to the 500th part of the whole, which is not worth regarding; and indeed if it were much greater, it would be of very little consequence, as it would be always the same in trying the same kind of air.

There are several contrivances which I use, in order to diminish the trouble of weighing the vessels; but I omit them, as the description would take up too much room.

The vessel A holds 282 grains of water, and is the quantity which I shall distinguish by the name of one measure. I have three bottles for mixing the airs in, with a measure B for the nitrous air adapted to each. The first bottle holds three measures, and the corresponding measure $1\frac{1}{4}$; the second bottle holds six, and the corresponding measure $2\frac{1}{4}$; and the third bottle holds 12, and the corresponding measure 5. The first bottle and measure is used in trying common air, or air not better than that; the two other in trying dephlogisticated air. The quantity of respirable air used, as was said before, is always the same, namely, one measure; consequently, in trying common air I use $1\frac{1}{4}$ measures of nitrous air to one of common; and in trying very pure dephlogisticated air I use five measures of nitrous air to one of the dephlogisticated. I believe there is no air so much dephlogisticated as to require a greater proportion of nitrous than that. The way by which I judge whether the quantity of nitrous air used is sufficient, is by the bulk of the two airs when mixed; for if that is not less

less than one measure, that is, than the respirable air alone, it is a sign that the quantity of nitrous air is sufficient; or that it is sufficient to produce the full diminution, unless it is very impure.

Though the quantity of respirable air used will be always nearly the same, as being put in by measure; yet it will commonly be not exactly so, for which reason the observed diminution will commonly require some correction: for example, suppose that the observed diminution was 2.353 measures, and that the quantity of respirable air was found to be .985 of a measure; then the observed diminution must be increased by $\frac{1}{1000}$ of the whole or .035, in order to have the true diminution, or that which would have been produced if the respirable air used had been exactly one measure; consequently, the true diminution is 2.388.

The method of weighing, described in p. 6. is that which I use in trying air much different in purity from common air; but in trying common air, I use a shorter method, namely, I do not weigh the vessel A at all, but only weigh the bottle M with the nitrous air in it; then mix the airs, and again weigh the same bottle with the mixture in it, and find the increase of weight. This, added to one measure, is very nearly the true diminution, whether the quantity of common air used was a little more or a little less than one measure. The reason of this is, that as the diminution produced on mixing common and nitrous air is only a little greater than the bulk of the common air, the bulk of the mixture will be very nearly the same, whether the bulk of the common air is a little greater or a little less than one measure: for example, let us first suppose, that the quantity of common air used is exactly one measure, and that the diminution of bulk on mixing is .08 of a measure,

B

then

then must the increase of weight of the bottle M, on adding the common air, be .08 of a measure. Let us now suppose, that the quantity of common air used is 1.02 of a measure; then will the diminution, on adding the common air, be $1.08 \times \frac{1.02}{1.00}$ or 1.1016 of a measure, and consequently the increase of weight of the bottle M. will be $1.1016 - 1.02$ or .0816 of a measure, which is very nearly the same as if the common air used had been exactly one measure.

In the second method of proceeding, or that in which the nitrous air is added to the respirable, I use always the same bottle, namely, that which holds three measures, and use always one measure of respirable air; and in trying common air use the same vessel A as in the first method; but for dephlogisticated air I use one that holds $3\frac{1}{2}$ measures.

In trying the experiment I first weigh the bottle M without any air in it, and then weigh it again with the respirable air in it, which gives the quantity of respirable air used. I next put the nitrous air into the vessel A, and weigh that and the bottle M together, and then having mixed the airs, weigh them again, which gives the diminution.

From what has been just said, it appears, that in this method of proceeding I use a less quantity of nitrous air in trying the same kind of respirable air than in the former; the reason of which is, that the same quantity of nitrous air goes further in phlogisticating a given quantity of respirable air in this than in the former method, as will be shewn further on.

In both these methods I express the test of the air by the diminution which they suffer in mixing; for example, if the diminution on mixing them is two measures and $\frac{3}{1000}$, I call its test 2.353, and so on.

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In the first method of proceeding I found, that the diminution was scarce sensibly less when I used one measure of nitrous air than when I used a much greater quantity; so that one measure is sufficient to produce the full diminution. I chuse, however, to use $1\frac{1}{4}$, for fear the nitrous air may be impure; $\frac{2}{3}$ ths of a measure of nitrous air produced about $\frac{1}{3}$, and $\frac{1}{4}$ ths of a measure about $\frac{2}{9}$ ths of the full diminution.

I found also, that there was no sensible difference in the diminution whether the orifice by which the air passed out of the vessel A into the bottle M was only $\frac{1}{3}$ th of an inch in diameter, or whether it was $\frac{1}{4}$ th of an inch; that is, whether the air escaped in smaller or larger bubbles. The diminution was rather less when the bottle was shook gently than when briskly; but the difference between shaking it very gently and as briskly as I could was not more than $\frac{1}{100}$ th of a measure. But if it was not shaken at all the diminution was remarkably less, being at first only .9; in about 3', indeed, it increased to .93, and after being shaken for about a minute it increased to .99; whereas, when the bottle was shaken gently, the diminution was 1.08 at first mixing, and did not increase sensibly after that time. The difference proceeding from the difference of time which the air took up in passing into the bottle was rather greater; namely, in some trials, when it took up 80'' in passing, the diminution was $\frac{5}{100}$ ths greater than when it took up only 22'', and about $\frac{2}{100}$ ths greater than when it took up 45'; in some other trials, however, the difference was less. It appears, therefore, that the difference arising from the difference of time which the air takes up in passing into the bottle is considerable; but, as with the same hole in the plate Dd it will take up always nearly the same time, and as it is easy adjusting the size of the hole, so as to make it take up nearly the

time we desire, the error proceeding from thence is but small. The time which it took up in passing in my experiments was usually about 50".

The difference proceeding from the difference of size of the bottle, and the nature of the water made use of is greater; for when I use the small bottle which holds three measures, and fill it with distilled water, the usual diminution in trying common air is 1.08; whereas, if I fill the bottle with water from my tub, the diminution is usually about .05 less. If I use the bottle which holds twelve measures, filled with distilled water, the diminution is about 1.15; and if I use the same bottle, filled with water from my tub, about 1.08.

The reason of this difference is, that water has a power of absorbing a small quantity of nitrous air; and the more dephlogisticated the water is, the more of this air it can absorb. If the water is of such a nature also as to froth or form bubbles on letting in the common air, the diminution is remarkably less than in other water.

The following table contains the diminution produced in trying common air in the bottle containing three measures, with several different kinds of water, and also the diminution which the same quantity of nitrous air suffered by being only shook in the same bottle, without the addition of any common air, tried by stopping the mouth of the bottle with my finger, and shaking it briskly for one minute, and afterwards for one minute more.

Dimi-

Diminution in trying common air.	Diminution on shaking ni- trous air for		
	one minute.	two minutes.	
1.099	.118	.122	Distilled water.
1.049	.083	.088	Water from tub.
1.036	.090	.098	Pump water.
1.062	.090	.099	Distilled water, in which a few drops of liver of sulphur were kept for a few days.
1.045	.052	.056	Distilled water impregnated with nitrous air, by keeping it with about $\frac{1}{4}$ of its bulk of ni- trous air for two days, and frequently shaking it.
.897	.082	.085	Water fouled by oak shavings. N. B. it frothed very much.

In general, the diminution was nearly as great with rain water as distilled water; but sometimes I have found rain water froth a good deal, and then the diminution was not much greater than by the water fouled with oak shavings.

This difference in the diminution, according to the nature of the water, is a very great inconvenience, and seems to be the chief cause of uncertainty in trying the purity of air; but it is by no means peculiar to this method, as I have found as great a difference in FONTANA's method, according as I have filled the tube with different waters*. But it shews plainly, how little all the experiments which have hitherto been made for determining the variations in the purity of the atmosphere can be relied on, as I do not know that any one before has been attentive to the nature of the water he has used, and the difference proceeding from the difference of waters is much greater than any I have yet found in the purity of air.

* I do not find that it makes much difference in FONTANA's method whether the water is disposed to froth or not; but the advantage which it has in that respect over this method is not of much consequence, as it is easy, finding water which will not froth.

The best way I know of obviating this inconvenience is to be careful always to use the same kind of water: that which I always use is distilled, as being most certain to be always alike. I should have used rain water, as being easier procured, if it had not been that this water is sometimes apt to froth, which I have never known distilled water do.

As I found that the power with which the distilled water I used absorbed nitrous air was greater at some times than others, which must necessarily make an error in the observation, I was in hopes that, by observing the quantity of nitrous air which the water absorbed in the same manner as in the preceding experiment, together with the heat of the water, as that also seems to affect the experiment, one might be able to correct the observed test, and thereby obviate the error which would otherwise arise from any little difference in the nature of the water employed. With this view I made the following experiment

I purged some distilled water of its air by boiling, and kept one part of it for a week in a bottle along with some dephlogisticated air, and shook it frequently; the other part was treated in the same manner with phlogisticated air. At the end of this time I found, by a mean of three different trials, that the test of common air tried with the first of these waters was 1.139, the diminution which nitrous air suffered by being shook 2' in it in the usual manner was .285. The test of the same air tried with the last of these waters was only 1.054, and the diminution of nitrous air only .090, the heat of the water in the tub and of the distilled waters being 45° . I then raised both the water of the tub and the distilled waters to the heat of 67° , and found that the test of the same air, tried by the first water, was then 1.100, and by the latter 1.044; and that the

the diminution of nitrous air was .235 by the first water, and .089 by the latter.

It should seem from hence, as if the observed test ought to be corrected by subtracting $\frac{1}{3}$ ths of the diminution which nitrous air suffers by being shaken in the water, and adding .007 for every 3° of heat above 0, as the foregoing trials will agree very well together, if they are corrected by this rule, and better than if corrected by any different rule, as will appear by the following table.

	Heat.	Diminution of nitrous air.	Observed test.	Correction for Diminu- tion.	Heat.	Corrected test.
Former water	{ 45	.285	1.139	.114	.030	1.055
	{ 67	.235	1.100	.094	.045	1.051
Latter water	{ 45	.090	1.054	.036	.030	1.048
	{ 67	.089	1.044	.036	.045	1.053

Though in all probability this correction will diminish the error proceeding from a difference in the nature of the distilled water employed, yet I have reason to think, that it will by no means entirely take it away; for which reason I do not in general make use of it. In almost all the trials, indeed, in which I have applied the correction, it has come out very nearly the same; which seems to shew, that there was no other difference in the absorbing power of the distilled water I employed, than what proceeded from its difference of heat. The above experiment, however, shews plainly, that distilled water is capable of a very great difference in this respect independent of its heat.

In the second method of proceeding, or that in which the nitrous air is added to the respirable, I found nearly the same difference.

difference in the diminution, according as the bottle was shaken briskly or gently, as in the former method: I found also nearly the same difference, or perhaps rather less, according to the nature of the water employed, only it seemed to be of not much consequence whether the water frothed or not; but there seemed to be much less difference in the diminution, according to the time which the air took up in passing into the bottle. The usual diminution on trying common air with different quantities of nitrous air, when distilled water was employed, was as follows:

Common air.	Nitrous air.	Diminution.
1. {	.6	.74
	.8	.88
	1.	.89
	1.5	.90

It appears, therefore, that $\frac{3}{4}$ ths of a measure of nitrous is sufficient to produce very nearly the full diminution. I chuse, however, always to use one measure. It appears also, that the diminution is always much less in this method than when the common air is added to the nitrous; as in that method it was before said, that the usual diminution was 1.08. The reason of this is, that when nitrous and common air are mixed together, the nitrous air is robbed of part of its phlogiston, and is thereby turned into phlogisticated nitrous acid, and is absorbed by the water in that state, and besides that, the common air is phlogisticated, and thereby diminished: so that the whole diminution on mixing is equal to the bulk of nitrous air, which is turned into acid, added to the diminution which the common air suffers by being phlogisticated. Now it appears, that when a small quantity of nitrous air comes in contact with a large quantity

quantity of common air, it is more completely deprived of its phlogiston, and is absorbed by the water in a more dephlogisticated state than when a small quantity of common air comes in contact with a large quantity of nitrous; consequently, in the second method, where small portions of nitrous air come in contact with a large quantity of common air, the nitrous air is more deprived of its phlogiston, and therefore a less quantity of it is required to phlogistificate the common air than in the first method, where small portions of common air come in contact with a large quantity of nitrous air; so that a less quantity of the nitrous air is absorbed in the second method than in the first. As to the common air, as it is completely phlogistified in both methods, it most likely suffers an equal diminution in both.

A clear proof that a less quantity of nitrous is required to phlogistificate a given quantity of common air in the second method than in the first, is, that if common air is mixed with a quantity of nitrous air not sufficient to completely phlogistificate it, the mixture will be more phlogistified if the nitrous air is added slowly to the common, as in the second method, than if the common air is added to the nitrous; and if the nitrous air is added slowly to the common, without being in contact with water, the mixture will be found to be still more phlogistified than in the second method, where the two airs are in contact with water at the time of mixing.

The following table contains the result of the experiments I have made on this subject.

First method.			Second method.			Nitrous air added slowly to common without being in contact with water.		
Nitrous air.	Bulk of mixture.	Test.	Nitrous air.	Bulk of mixture.	Test.	Nitrous air.	Bulk of mixture.	Test.
.716	.856	.244	.635	.849	.137	.294	.836	.337
.474	.915	.513	.430	.867	.352			
			.280	.930	.599			

The two first sets of experiments were not tried with the apparatus above described, as that held too small a quantity, but with another upon the same principle. The last set was tried by the apparatus represented in fig. 4. where A is a bottle containing nitrous air, inverted into the tub of water DE; B is a bottle with a bent glass tube C fitted to its mouth. This bottle is filled with common air, without any water, and is first slightly warmed by the hand; the end of the glass tube is then put into the bottle of nitrous air, as in the figure; consequently, as the bottle B cools, a little nitrous air runs into it, which, by the common air in it, is deprived of its elasticity, so that more nitrous air runs in to supply its place. By this means the nitrous air is added slowly to the common without coming in contact with water, till the whole of the nitrous air has run out of the bottle A into B; then, indeed, the water runs through the glass tube into B, to supply the vacancy formed by the diminution of the common air.

It appears from the foregoing table, that a quantity of nitrous air, used in the first method, does not phlogistificate common air more than three-fourths of that quantity used in the second way does, and not so much as half that quantity used in the third way: so that we may safely conclude, that it is this circumstance of the nitrous air going further in phlogistificating

cating common air in some circumstances than others, which is the cause that the diminution in trying the purity of air by the nitrous test is so much greater in some methods of mixing them than in others.

From what was said in p. 119. it should seem as if the second method was more exact than the first, as the error proceeding from the air employing more or less time in passing into the bottle was found to be less, and that proceeding from a difference in the water, and from the bottle being shaken more or less strongly was not greater. I, however, have found, that the trials of the same air on the same day have commonly differed more when made in this manner than in the first; for which reason, and because in trying common air the first method takes up the least time, I have commonly used that.

It should be observed, that in trying dephlogisticated air by the first method it is convenient to use different bottles, according to the different purity of the air; and the same air will appear purer, if tried by a larger bottle than by a smaller. For example, if its test, tried by the large bottle, comes out 2.54, it will appear not more than 2.44, if tried by the middle bottle; and, in like manner, if its test by the middle bottle comes out 1.11, it will appear to be about 1.08, if tried by the least bottle; for this reason it is right always to set down which bottle it is tried by.

I think I may confidently assert, that either of the above methods are considerably more accurate than FONTANA's, supposing the experiment to be made exactly in his manner, that is, determining the quantities by measure. But, in order to judge which method of mixing the airs is most exact, it was necessary to determine the quantities in his method also by weight, as otherwise it would be uncertain whether my method of

mixing the airs is really better than his, or whether the apparent greater exactness proceeds only from the superiority of weighing above measuring: for this reason I made some experiments in which common and nitrous air were mixed in his manner, except that I used only one measure of each, as Dr. INGEN-HOUZ did, and that the nitrous air was put up first, the true diminution being determined by weight, by first weighing the tube under water with the nitrous air in it, and then adding the common air, and weighing the tube again under water. It was unnecessary, for the reasons given in p. 110. and 112. to determine the quantity of either the nitrous or common air by weight. My reason for this variation was, that it afforded a much easier method of determining the quantities by weight, was less trouble, and, I believe, must be at least as exact: for I have always found, that the experiments made with the Abbé FONTANA's apparatus, in which I used only one measure of each air, agreed better together than those in which I used two of common, and added the nitrous air by one at a time; and I imagine it can be of no signification whether the nitrous or common air is put in first, as I cannot perceive the diminution to be sensibly greater in one of those ways than the other*.

* It is not extraordinary, that in this method the diminution is just the same whether the common or nitrous air is put up first, notwithstanding that in mine it is very different; since in this method the two airs mix in the same manner whichever is put up first: whereas in mine, the manner in which they mix is very different in those two cases; as in one, small portions of common air come in contact with large portions of the nitrous; and in the other, small portions of nitrous air come in contact with large portions of common air.

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From the result of these experiments I am persuaded, that my method of mixing the airs is really rather more accurate than FONTANA's, as in trying the same bottle of air six or seven times in my method the different trials would not often differ more than $\frac{1}{100}$ dth part, and very seldom more than $\frac{1}{100}$ dth; whereas in his there would commonly be a difference of $\frac{1}{100}$ dth, and frequently near twice that quantity, though I endeavoured to be as regular as I could in my manner of trying the experiment. My method also certainly requires less dexterity in the operator than his.

It is of much importance towards forming a right judgement of the degree of accuracy to be expected in the nitrous test, to know how much it is affected by a difference in the nitrous air employed. Now it must be observed, that nitrous air may differ in two respects; first, it may vary in purity, that is, in being more or less mixed with phlogisticated or other air; and, secondly, it is possible, that out of two parcels equally pure one may contain more phlogiston than the other. If it differs in the second respect, it will evidently cause an error in the test, in whatever proportion it is mixed with the respirable air; but if it differs only in the first respect, it will hardly cause any sensible error, unless it is more than usually impure, provided care is taken to use such a quantity as is sufficient to produce the full diminution. This has been observed by the Abbé FONTANA, and agrees with my own experiments; for the test of common air tried in my usual method, with some nitrous air which had been debased by the mixture of common air, came out only 18 thousandths less than when tried with air of the best quality, though this air was so much debased that the diminution, on mixing two parts of this with five of common, was one-sixth part less than when good nitrous air was

was employed; which shews, that the error proceeding from the difference of purity of the nitrous air is much less when it is used in the full quantity than in a smaller proportion; and also shews, that if it is used in the full quantity it can hardly cause any sensible error, unless it is more impure than usual. One does not easily see, indeed, why it should cause any error; for no reason appears why the mixture of phlogisticated or other air, not absorbable by water, and not affected by respirable air, should prevent the nitrous air from diminishing and being diminished by the respirable air in just the same manner that it would otherwise be. It must be observed, however, that if the nitrous air is mixed with fixed air, it will cause an error, as part of the fixed air will be absorbed by the water while the test is trying; for which reason care should be taken that the nitrous air should not be much mixed with this substance, which it will hardly be, unless either the metal it is procured from is covered with rust; or unless the water in which it is received contains much calcareous earth suspended by fixed air, as in that case, if any of the nitrous acid comes over with the air, it will dissolve the calcareous earth, and separate some fixed air.

In order to see whether it is possible for nitrous air to differ in the second respect, I procured some from quicksilver, copper, brass, and iron, and observed the test of the same parcel of common air with them, on the same day, making four trials with each, when the difference between the tests tried with the three first kinds of air was not greater than might proceed from the error of the experiment; but those tried with the air from iron were $\frac{1}{800}$ ths greater than the rest. I then took the test of some more common air with them in the same manner, only using four parts of common to one of nitrous air, when

when the tests tried with the air from iron came out smaller than the rest by not less than $\frac{1}{1000}$ ths. It should seem, therefore, from these experiments, that the nitrous air procured from iron, besides being much more impure than the others, differs from them also in the second respect; that is, that the pure nitrous air in it contains rather less phlogiston than that in the others: whence it happens, that a greater quantity is necessary to phlogisticate a given portion of common air, and consequently that the diminution is greater when a sufficient quantity of it is used, though with a less proportion the diminution is much less than with other nitrous air, on account of its greater impurity. As for the air procured from the three other substances, I cannot be sure that there is any difference between them. The nitrous air I always use is made from copper, as it is procured with less trouble than from quicksilver, and I have no reason to think it more likely to vary in its quality.

During the last half of the year 1781, I tried the air of near 60 different days, in order to find whether it was sensibly more phlogisticated at one time than another; but found no difference that I could be sure of, though the wind and weather on those days were very various; some of them being very fair and clear, others very wet, and others very foggy.

My way was to fill bottles with glass stoppers every now and then with air from without doors, and preserve them stopped and inverted into water, till I had got seven or eight, and then take their test; and whenever I observed their test, I filled two bottles, one of which was tried that day, and the other was kept till the next time of trying, in order to see how nearly the test of the same air, tried on different days, would agree. The experiment was always made with distilled water, and care was
always

always taken to observe the diminution which nitrous air suffered by being shaken in the water, as mentioned in p. 115. The heat of the water in the tub also was commonly set down. Most of the bottles were tried only in the first method; but some of them were also tried by the second, and by the method just described in the manner of FONTANA.

The result was, that the test of the different bottles tried on the same day never differed more than .013, and in general not more than half that quantity. The test, indeed, of those tried on different days differed rather more; for taking a mean between the tests of the bottles tried on the same day, there were two of those means which differed .025 from each other; but, except those two, there were none which differed more than .013. Though this difference is but small, yet as each of these means is the mean of seven or eight trials, it is greater than can be expected to proceed from the usual errors of the experiment. This difference also is not much diminished by correcting the observations on account of the heat and absorbing power of the water, according to the rule in p. 118. This might incline one to think, that the parcels of air examined on some of those days of trial were really more dephlogisticated than the rest; but yet, I believe, that they were not: for whenever there was any considerable difference between the means of two successive days of trial, there was nearly the same difference between the tests of the two bottles of the very same air tried on those two days. For example, the mean of the trials on July 7. was .016 less than that of those on the 15th of the same month; but then the test of the air caught and tried on the 7th was equally less than that of the air of the same day tried on the 15th; which shews, that this difference between the means of those two days was not owing to

to the parcels of air tried on the former day being really more dephlogisticated than those tried on the latter, but only to some unperceived difference in the manner of trying the experiment; or else to some unknown difference in the nature of the water or nitrous air employed. A circumstance which seems to shew that it was owing to the first of these two causes is, that it frequently happened, that on those days in which the tests taken in the first method came out greater than usual, those taken in FONTANA's manner, or in the second method, did not do so; the trials, however, made in these two methods were too few to determine any thing with certainty. On the whole there is great reason to think, that the air was in reality not sensibly more dephlogisticated on any one of the sixty days on which I tried it than the rest.

The highest test I ever observed was 1.100, the lowest 1.068, the mean 1.082.

I would by all means recommend it to those who desire to compare the air of different places and seasons, to fill bottles with the air of those places, and to try them at the same time and place, rather than to try them at the time they were filled, as all the errors to which this experiment is liable, as well as those which proceed from small differences in the manner of trying the experiment, as those which proceed from a difference in the nature of the water and nitrous air, will commonly be much less when the different parcels of air are tried at the same time and place than at different ones; provided only, that air can be kept in this manner a sufficient time without being injured, which I believe it may, if the bottles are pretty large, and care is taken that they, as well as the water used in filling them with air, are perfectly clean. I have tried air kept in the abovementioned manner for upwards of three-quarters of a

year in bottles holding about a pint, which I have no reason to think was at all injured; but then I have tried some kept not more than one-third part of that time which seemed to have been a little impaired, though I do not know what it could be owing to, unless it was that the bottles were smaller, namely, holding less than one-fourth of a pint, and that in all of them, except two, which were smaller than the rest, the stopper which, however, fitted in very tight, was tied down by a piece of bladder.

I made some experiments also to try whether the air was sensibly more dephlogisticated at one time of the day than another, but could not find any difference. I also made several trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the same day, and nearly at the same hour, at Marlborough-street and at Kensington. The result was, that sometimes the air of London appeared rather the purest, and sometimes that of Kensington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the smoke.

It is very much to be wished, that those gentlemen who make experiments on factitious airs, and have occasion to ascertain their purity by the nitrous test, would reduce their observations to one common scale, as the different instruments employed for that purpose differ so much, that at present it is almost impossible to compare the observations of one person with those of another. This may be done, as there seems to be so very little difference in the purity of common air at different

ferent times and places, by assuming common air and perfectly phlogisticated air as fixed points. Thus, if the test of any air is found to be the same as that of a mixture of equal parts of common and phlogisticated air, I would say, that it was half as good as common air; or, for shortness, I would say, that its standard was $\frac{1}{2}$: and, in general, if its test was the same as that of a mixture of one part of common air and x of phlogisticated air, I would say, that its standard was $\frac{x}{1+x}$. In like manner, if one part of this air would bear being mixed with x of phlogisticated air, in order to make its test the same as that of common air, I would say, that it was $1+x$ times as good as common air, or that its standard was $1+x$; consequently, if common air, as Mr. SCHEELÉ and LA VOISIER suppose, consists of a mixture of dephlogisticated and phlogisticated air, the standard of any air is in proportion to the quantity of pure dephlogisticated air in it. In order to find what test on the Eudiometer answers to different standards below that of common air, all which is wanted is to mix common and perfectly phlogisticated air in different proportions, and to take the test of those mixtures; but in standards above that of common air, it is necessary to procure some good dephlogisticated air, and to find its standard by trying what proportion of phlogisticated air it must be mixed with, in order to have the same test as common air, and then to mix this dephlogisticated air with different proportions of phlogisticated air, and find the test of those mixtures*.

On

* The rule for computing the standard of any mixture of dephlogisticated and phlogisticated air is as follows. Suppose that the test of a mixture of D parts of dephlogisticated air with P of phlogisticated air is the same as that of common

On this principle I found the standard answering to different tests on both my Eudiometers, and also on FONTANA's, to be as follows:

Standard.	Test by first method.	Test by second method.	Test by FONTANA abridged.				Total diminution.
			1	2	3	4	
4.8	5.02	3.62	.73	.44	.13	1.02	3.98
3.61	3.72	2.70	.75	.49	1.00	—	3.00
2.39	2.55 by large bottle	1.87	.76	.96	1.92	—	2.08
	2.45 by middle bottle						
1.00	1.11 by middle bottle	.89	1.00	—	—	—	1.00
	1.08 by least bottle						
.75	.81	.69	1.23	—	—	—	.77
.5	.57	.51	1.45	—	—	—	.55
.25	.32	.31	1.66	—	—	—	.34
.0	.07	.08	1.94	—	—	—	.06

Standard.	Test by FONTANA's method.							Total diminution.
	1	2	3	4	5	6	7	
4.8	1.75	1.43	1.11	.78	.46	.21	1.18	7.82
3.61	1.75	1.46	1.17	.89	1.16	2.13	—	5.87
2.39	1.76	1.50	1.25	2.06	—	—	—	3.94
1.	1.81	2.12	3.12	—	—	—	—	1.88
.75	1.82	2.54	—	—	—	—	—	1.46
.5	1.98	2.94	—	—	—	—	—	1.06
.25	2.42	3.39	—	—	—	—	—	.61
.0	2.91	—	—	—	—	—	—	.09

The phlogisticated air used in these experiments was procured by means of liver of sulphur.

air, then is the standard of the dephlogisticated air $\frac{D+P}{D}$. Let now δ parts of this dephlogisticated air be mixed with ϕ parts of phlogisticated air, the standard of the mixture will be $\frac{D+P}{D} \times \frac{\delta}{\delta+\phi}$.

The

The trials, called FONTANA abridged, were made in the Abbé FONTANA's manner, except that only one measure of respirable air was used, the nitrous air being added by one measure at a time, as usual. The column marked 1 at top is the bulk of the mixture after one measure of nitrous air was added; that marked 2, its bulk after two measures were added, and so on.

It must be observed, that in these experiments a considerable diminution took place in taking the test of the unmixed phlogisticated air, or that whose standard is marked 0 in the table; but, notwithstanding this, the air, as far as I could perceive, was perfectly phlogisticated, the diminution being caused merely by the absorption of the nitrous air by the water. What shews this to be the case is, that if common and nitrous air are mixed in such proportions as that the nitrous should be predominant, so as to be considerably diminished by the mixture of common air, this mixture will produce as great a diminution with nitrous air as the phlogisticated air used in these experiments; and if plain nitrous air is added to nitrous air, the diminution is still greater. This shews, that a considerable diminution is produced by mixing perfectly phlogisticated air with nitrous air, and also that air may be perfectly phlogisticated by liver of sulphur.

These experiments also shew the necessity of using such a quantity of nitrous air as is sufficient to produce the full diminution, in order to form a proper estimate of the goodness of air; for if the quantity of nitrous air is much less than that, the air you try will appear very little better than air of a much inferior quality. For example, if in taking the test of very good dephlogisticated air, only an equal bulk of nitrous air is used,

used, it will appear very little better than a mixture of equal parts of this and phlogisticated air; and if twice that quantity of nitrous air is used, it will appear very little better than a mixture of three parts of this air with one of phlogisticated. Another great advantage of using the full quantity of nitrous air is, that thereby the error arising from any difference in its purity is very much diminished.

Perfectly phlogisticated air may be conveniently procured by putting some solution of liver of sulphur into a bottle of air well stopped, and shaking it frequently till the air is no longer diminished, which, unless it is shaken very frequently, will take up some days. Care must be taken, however, to loosen the stopper now and then, so as to let in air to supply the place of the diminished air. In order to know when the air is as much diminished as it can be, the best way is, when the air is supposed to be nearly phlogisticated, to place the bottle with its mouth under water, still keeping it stopped, and to loosen the stopper now and then, while under water, so as to let in water to supply the place of the diminished air, by which means the alteration of weight of the bottle shews whether the air is diminished or not. If the solution of liver of sulphur is made by boiling together fixed alkali, lime, and flowers of sulphur, which is the most convenient way of procuring it, the air phlogisticated by it will be perfectly free from fixed air: whether it will be so if the liver of sulphur is made without lime, I am not sure.

A still more convenient way, however, of procuring phlogisticated air is by a mixture of iron filings and sulphur; and, as far as I can perceive, the air procured this way is as completely phlogisticated as that prepared by liver of sulphur.

Where

Where the impurities mixed with the air have any considerable smell, our sense of smelling may be able to discover them, though the quantity is vastly too small to phlogistificate the air in such a degree as to be perceived by the nitrous test, even though those impurities impart their phlogiston to the air very freely. For instance, the great and instantaneous power of nitrous air in phlogistificating common air is well known; and yet ten ounce measures of nitrous air, mixed with the air of a room upwards of twelve feet each way, is sufficient to communicate a strong smell to it, though its effect in phlogistificating the air must be utterly insensible to the nicest Eudiometer; for that quantity of nitrous air is not more than the $\frac{1}{140000}$ th part of the air of the room, and therefore can hardly alter its test by more than $\frac{1}{140000}$ or $\frac{1}{47000}$ th part. Liver of sulphur also phlogistificates the air very freely, and yet the air of a room will acquire a very strong smell from a quantity of it vastly too small to phlogistificate it in any sensible degree. In like manner it is certain, that putrifying animal and vegetable substances, paint mixed with oil, and flowers, have a great tendency to phlogistificate the air; and yet it has been found, that the air of an house of office, of a fresh painted room, and of a room in which such a number of flowers were kept as to be very disagreeable to many persons, was not sensibly more phlogisticated than common air. There is no reason to suppose from these instances, either that these substances have not much tendency to phlogistificate the air, or that nitrous air is not a true test of its phlogistification, as both these points have been sufficiently proved by experiment; it only shews, that our sense of smelling can, in many cases, perceive infinitely finaller alterations in the purity of the air than can be perceived.

ceived by the nitrous test, and that in most rooms the air is so frequently changed, that a considerable quantity of phlogisticating materials may be kept in them without sensibly impairing the air. But it must be observed, that the nitrous test shews the degree of phlogistication of air, and that only; whereas our sense of smelling cannot be considered as any test of its phlogistication, as there are many ways of phlogisticating air without imparting much smell to it; and, I believe, there are many strong smelling substances which do not sensibly phlogisticate it.



very disagreeable to many. There is no reason to suppose that these instances, either that these substances have not much tendency to phlogisticate the air, or that nitrous air is not a true test of its phlogistication, as both these points have been sufficiently proved by experiment; it only shews, that our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity of the air than can be perceived



Fig. 4.

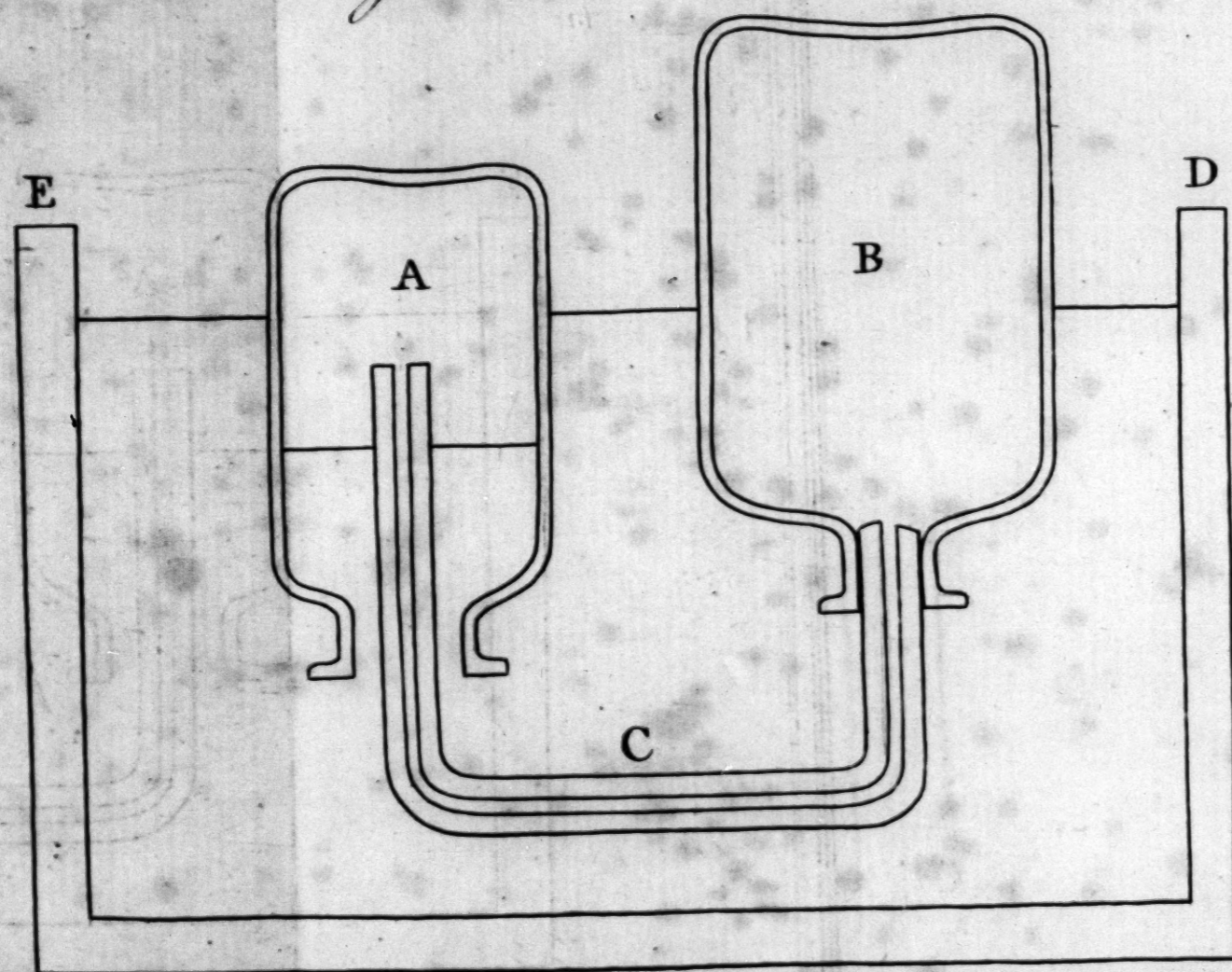


Fig. 1.

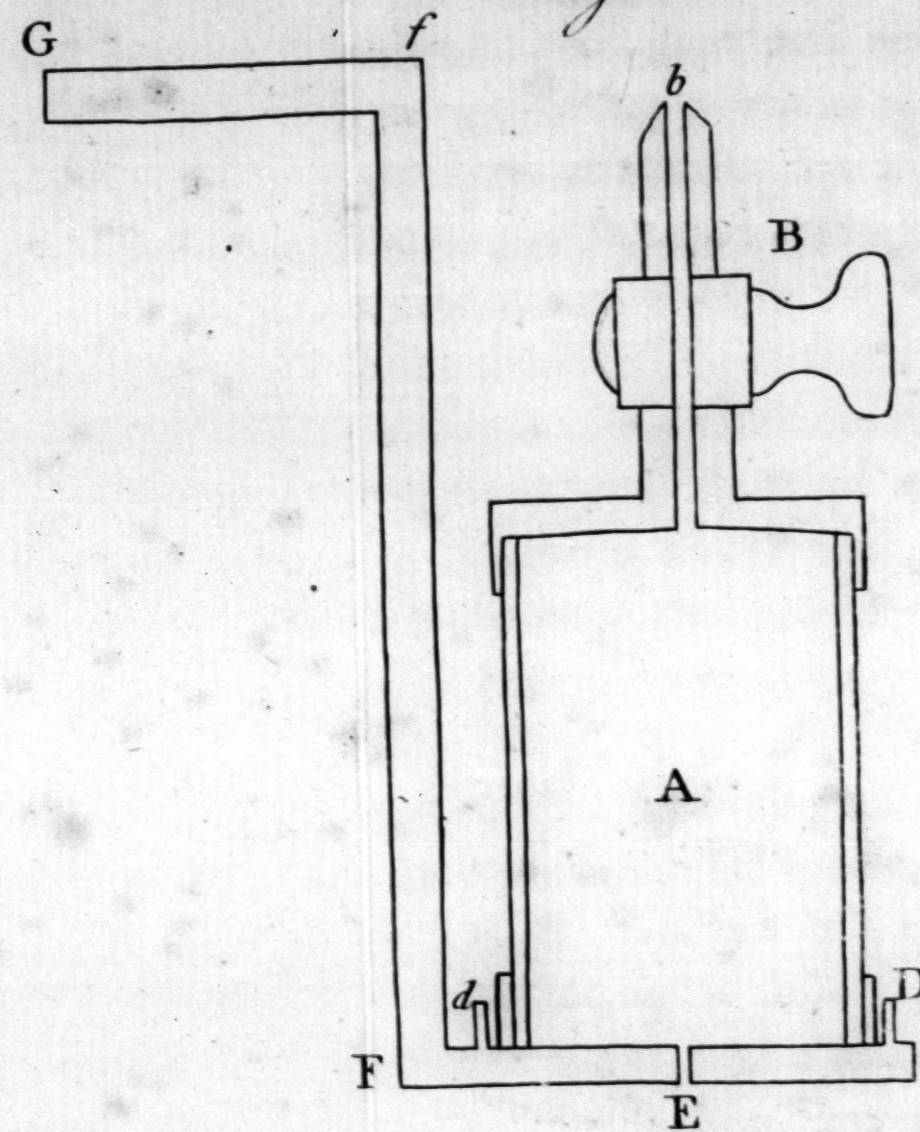


Fig. 2.

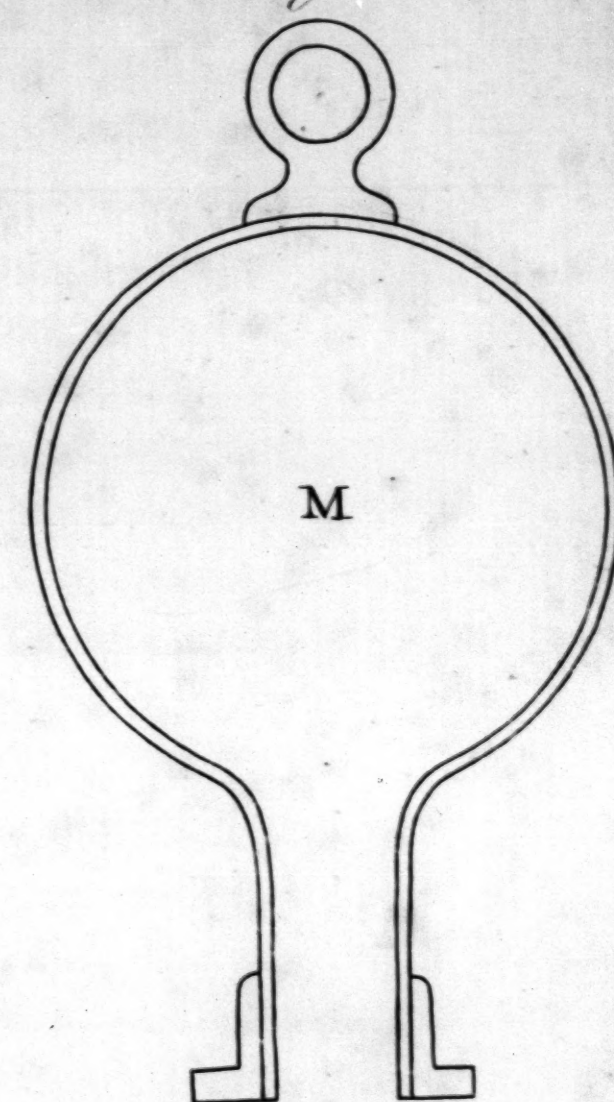


Fig. 3.

